

Dear reviewer #1,

Thank you very much for your valuable comments. The following is our response (in blue) to your comments.

(1) The paper says that it is comparing two sources for an O<sup>+</sup> enhancement at substorm onset: direct injection from the ionosphere or energization of a pre-existing O<sup>+</sup> ions. This reviewer thinks that this model cannot test the direct injection hypothesis. If direct injection were occurring, there would have to be some completely different physics occurring in the ionosphere to accelerate these 100 keV ions out, which is clearly not included in the model. This hypothesis can really only be tested through observation. The authors should change their approach by saying that they are testing whether the observed O<sup>+</sup> can be explained within the standard model (ie. outflow from the auroral regions transported into the tail and from there into the ring current). That they can explain the observations with the standard model means that the direct injection hypothesis is not needed, but it doesn't prove that it doesn't occur. It just doesn't occur in their model.

We agree with your view here. In the Summary section, we reword point (4) to only say that the O<sup>+</sup> bursts seem during substorms can be explained by non-adiabatic energization of pre-existing O<sup>+</sup>, without the contribution from direct injection of ionospheric ions.

(2) The discussion does a better job of putting the work into the context of the previous work than the introduction. Some of the discussion should be brought up to the front to put the work in context from the beginning. For example, in the introduction, the Mitchell et al. [2003] work is cited showing that energetic O<sup>+</sup> increases more than H<sup>+</sup>, implying that this is a key first measurement. But the discussion starts out by saying "It is well known that the O<sup>+</sup>/H<sup>+</sup> ratio increases during magnetic storms and substorms." In particular Daglis et al. 1996 showed the rapid increase in O<sup>+</sup> energy density at substorm onset. Also Moebius et al., 1987, showed that the ion spectra become harder at substorm onset, with O<sup>+</sup> being more energized than H<sup>+</sup>. That this is well known (and the associated references) should be put in the introduction, with the IMAGE results adding the additional information of the approximate spatial region where the increase occurs. Similarly the suggestion of non-adiabatic acceleration during dipolarization is credited to Delcourt [2002] here. But Mauk, 1986 was the first to show how the O<sup>+</sup> is accelerated non-adiabatically with the "convection surge" mechanism. He is cited later, but not in this introduction.

We have referenced the papers by Moebius et al, Daglis and Axford in the Introduction (p. 3, line 6-9). We do not reference Mauk (1986) here as you suggested because that work has assumed conservation of the first adiabatic invariant. What is meant by "nonadiabatic" in the Mauk study is the violation of the second adiabatic invariant which is different from what is considered in our study.

(3) This reviewer found the results for the auroral wind outflow parameters surprising. These results turn out to be key in determining where the O<sup>+</sup> ends up in the tail, so they should be checked for reasonableness. Using physical parameters within the model is a good idea, but only if the results agree with observations. What has actually been observed in terms of outflow and E-parallel? In particular, have the largest E-parallel flows been observed in the noon-to-dusk region? Why is there essentially no E-parallel in the nightside auroral region, where I think it is frequently observed? How do the outflow parameters compare with the statistical results of Yau et al. 1997?

In indeed at the earlier times in our simulation, the maximum  $E_{||}$  is found on the nightside and then drift to noon-to-dusk at the time we show in Figure 3, lower panels. We have added discussion on this at the beginning of page 11 in the revised manuscript. We have compared our O<sup>+</sup> and H<sup>+</sup> fluence (particle/second) with the statistical results of Yau et al. Our simulated O<sup>+</sup> flux is higher than, but in the same order of magnitude, that of Yau et al. Our H<sup>+</sup> fluence is in the range of Yau's. We have added the discussion of this comparison in the second paragraph of page 11.

Even it is not suggested by the reviewers, we add a figure (Figure 7) on the potential drop across the CRCM polar boundary at the ionosphere. This figure will provide information on how strong the overall convection would be during this event.

Thanks again for your careful reading of the paper and your constructive comments. We hope the revised manuscript is acceptable to you.

Dear reviewer #2,

Thank you very much for your positive reception of this paper. Your suggestions and comments are valuable in improving the quality of the paper. Below please find our responses to your comments (in blue).

1. Page 7, Lines 3-5.

There is a possibility that the continuous increase of hydrogen emission is caused by motion of the IMAGE satellite, since the altitude of the satellite was decreasing during 1230-1310 UT. Removal of this effect (i.e., altitude-corrected ENA flux) will be needed to examine if the increase is due to the enhanced convection.

Intensity ( $\text{cm}^2 \text{ sr s keV}^{-1}$ ) increases with decreasing distance when the source is smaller than the pixel. In the extreme case where the source can be considered a point-source in the pixel, the intensity goes like  $1/r^2$ . As the source extent grows the exponent in the denominator comes closer to zero. For these HENA images the ring current source is much larger than the pixel size and therefore intensity does not vary with distance. Low-altitude emissions (coming from within the limb of the Earth), can be considered point-like. However we are only concerned with the "high-altitude" emissions that are far away from the limb. We have added discussion at the end of Section 2 to clarify that the gradual increase in hydrogen cannot be attributed to the decreasing distance of the satellite to the source region.

2. Section 4.1-4.2 and Figures 6-7.

(a) Section 4.1 focuses on the non-adiabatic acceleration of  $\text{O}^+$  ions, in which the first invariant (magnetic moment) is violated. The bottom panel of Figure 6 clearly demonstrates a change of the first invariant. No description about the second invariant (or acceleration in the parallel direction) is found in this section. This may lead readers to consider that  $\text{O}^+$  ions are preferentially accelerated in the perpendicular direction. However, in section 4.2 and Figure 7, noticeable anisotropy of  $\text{O}^+$  ions can be found in the parallel direction. It is interpreted as a result of the centrifugal acceleration or the violation of the second invariant. This will confuse readers. I guess your point is that both acceleration mechanisms are effective, but the acceleration in the parallel direction is more dominant, causing the anisotropy. I suggest rewriting these sections as the point becomes clearer.

Thanks for pointing out this potential confusion. We have picked another ion trajectory to show in the paper. The new one, actually is a more typical one, has significant energization in parallel direction. We have also added discussion (p.15-16) on the violation of the second invariant when describing Figure 6.

(b) There are a few observational studies showing that ions are predominantly accelerated in the perpendicular direction to the magnetic field during dipolarization at  $r=8.8 \text{ Re}$  [Lui, JGR, p. 13067, 1996; see Figure 7] and  $X=-10.5 \text{ Re}$  [Nosé et al., JGR, p. 7669, 2000; see Figure 6]. Sánchez et al. [GRL, p. 177,

1993] performed a numerical simulation and showed that the pitch angle distribution shifts toward 90 degree during a non-adiabatic dipolarization (see the bottom panel of their Figure 3). The discrepancy of the pitch angle distribution between these previous studies and the present study might stem from a spatial and time scale of field reconfiguration. Field reconfiguration derived from the MHD simulation is expected to be rather global and gradual than that actually observed in the plasma sheet, causing weaker non-adiabatic acceleration (or less violation of the magnetic moment) and creating the more field-aligned pitch angle distribution. It would increase scientific significance of this paper to refer to these previous studies and discuss what caused the different results.

Thanks for the comments and references. We also found previous works that have shown field aligned distribution during substorms. Therefore we feel that there is no conclusive preference on pitch angle distribution (PAD) of substorm ions. The PAD may vary case by case. We have added a paragraph in the Discussion section (page 22, 2<sup>nd</sup> paragraph) to talk about this PAD issue. We agree with your point that fast substorms give perpendicular PAD and slow substorms field aligned.

3. Page 17, Lines 7-9.

It is clearly demonstrated in Figure 8 that O<sup>+</sup> energy in the ring current region is more strongly increased than H<sup>+</sup> energy. Is this strong enhancement of O<sup>+</sup> energy a result of energy increase at boundary of  $r=8$  Re? In other words, is the strong acceleration of O<sup>+</sup> ions more effective in the plasma sheet than in the ring current? Readers may be curious to see if O<sup>+</sup> ions still gain more energy than H<sup>+</sup> in the ring current region during the substorm.

We have looked at the H<sup>+</sup> and O<sup>+</sup> flux evolutions in the ring current region during the event. They have similar structures and configurations. Therefore we conclude that the difference in total energy is coming from different energy influx at the outer boundary. We have added a sentence on page 18, line 11-13, to clarify this point.

#### Minor Comments

1. Page 3, Line 4 from the bottom - Page 4, Line 1.

I could not understand the logical flow from the first sentence to the second sentence. Moreover the point of this paper is that field reconfiguration at substorm works differently between ion species rather than between sources of ions. (Note that ionospheric plasma contains H<sup>+</sup>.)

What we want to say here is that since solar wind and ionospheric ions take different paths to the ring current region, they experience different energization along their way. We have added, in the revised manuscript, a statement (page 4, line 3-5) to explain this.

2. Page 14, Line 9.

The pressure peak seems to be located at closer than 15 Re (maybe around 10 Re). We have changed it to 12 RE (page 15, line 3).

3. Page 14, Line 11.

Should read: "However, the O+ enhancement is stronger ...".  
Fixed.

4. Page 16, Line 11.  
Should read: "Delcourt [2002]".  
Fixed.

Even it is not suggested by the reviewers, we add a figure (Figure 7) on the potential drop across the CRCM polar boundary at the ionosphere. This figure will provide information on how strong the overall convection would be during this event.

Thanks again for your careful reading of the paper and the good scientific insight you have provided. We hope the revised manuscript is acceptable to you.